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Title	Comparing mental health services use between children and youth in military
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Reviewer 1	Dr. Christopher Perlman
Institution	Faculty of Applied Health Sciences, University of Waterloo, Waterloo, Ont.
General comments (author response in bold)	<p>This is an interesting study that has implications for understanding patterns of mental health service utilization among military families. It is a well designed study and the manuscript is generally well-written. Below are a number of questions and comments for the authors to consider.</p> <p>1. In the list of OHIP billing codes and CIHI/NACRS codes it sees that indicators or intentional self-harm were not included. Given that these are not mental disorders one could understand their exclusion. However, was there consideration for including such indicators for ED visits? Perhaps as a future research opportunity, the differences in the most responsible condition for the visits could be explored (this was not included in the recommendations for future research).</p> <p>We followed standard ICES recommendations for identifying mental health related outpatient and inpatient service use, which only search for relevant ICD codes within the “main” or “primary” diagnosis variable. Self-harm diagnoses can only be recorded in the supplementary diagnosis variables in NACRS. We agree that deliberate self-harm is an interesting, separate study and plan to publish those findings separately. We also agree that describing the reasons for mental health service use is of interest and have another manuscript under review that compares the reasons for outpatient visits. We have clarified this in the future recommendations section “Canadian studies defining the underlying burden of mental illness and needs of military families and healthcare experiences are needed and could include a study outlining differences in the most responsible condition for mental health-related visits.”</p> <p>2. It would have been interesting to know more about the context of the military families (and the population cohorts as well). In particular, we know that mental health of children and youth are strongly related to the mental health of their parents. Although burdensome, the study could have been strengthened by matching based on family utilization of OHIP and hospital mental health services. Why was this not included in the analysis?</p> <p>Matching of parents to children is not possible using administrative data at ICES. Mothers may be matched to children if they were born in Ontario, using a combination of data sources including hospital discharge abstracts. However, in this study, the military-connected children and youth were relocating to Ontario and many may have been born out of province. In addition, the mental health of serving parents would not be estimable because military members receive their healthcare outside the province’s jurisdiction and so we could not measure their mental health-related care.</p> <p>3. To the point above, it is not clear whether siblings were considered in the analysis (both from the military and non-military children and youth). Would the</p>

inclusion of siblings bias the results in some way? In the methods the authors should discuss whether siblings were included within the methods section and the implications of this on the interpretation of the findings in the discussion.

Siblings within military families would have been included in the military-connected cohort, while siblings were not purposively selected in the general comparison cohort. This is due to limitations in the ICES data in connecting family units (as described above). We have added this limitation “We were unable to account for clustering within military families; siblings from military families were included but we could not match them to siblings from the general population”.

4. The use of a count variable for defining intensity of service utilization makes sense. However, a hierarchical structure could have also been considered to define intensity. For instance, ED use or inpatient stays would imply a greater intensity of service use and thus, potentially a more severe mental health concern. This could have been further differentiated by considering short-stay OMHRS admissions (stays of less than 72 hours) vs full admissions (stays of 72 hours or longer). Was there any consideration for such a hierarchical structure to service intensity?

We agree that different types of health service use, in particular hospitalization and ED use suggest more acute and/or severe mental health needs, while outpatient services suggest less acute/less severe mental health needs. We did not consider separating hospitalizations based on LOS given how few hospitalizations occurred in either cohort.

5. The authors should clarify how they dealt with transfers between service settings when constructing counts of services (e.g., ED to OMHRS, OMHRS community hospital to OMHRS specialty hospital). There are instances where these transfers would indicate a single episode of care. The concern is double counting of single episodes. If there were differences in the severity of the condition between the comparison groups leading to the ED or hospital visits one group may have been more likely to experience such transfers.

We agree that episodes of care are typically defined across inpatient settings, rather than counting each separate admission as a new hospitalization. However, given the relative rarity of this event and how few individuals experiences more than one hospitalization, we did not further classify these hospitalizations into episodes of care. If we were moving forward with an analysis of hospitalizations in more detail, we would consider doing as suggested.

6. In the discussion the authors note on p.8 “The findings of this study in Canada are novel and provide unique information to help develop provincial and defense health policy to support the delivery of services to military families.” This is an important but the discussion doesn’t describe how the findings could be used to develop such policies. A few examples would be helpful, perhaps outlining the specific reforms necessary.

Additions to the discussion have been made to address this comment. Given the number of changes to this section, the text has not been copied over to the reviewer response document.

7. It is not clear how the findings presented in the study support the conclusions

	<p>being made. The conclusion read more as a rationale for the importance of the research and discussion of the implications of the work. The authors may consider revising the conclusions to be more specific to the findings of the study.</p> <p>The conclusions have been changed to: “Canadian military families are generally strong and adaptive; however, relocations may disrupt access to usual supports, including mental health care. If and when children and youth in a military family needs help, it’s critical that there are pathways in place to ensure that the mental health and wellbeing of children and youth are not affected negatively as a consequence of their parent’s military service. Provincial policies aimed at increasing access to mental health specialists for children and youth in military families alongside targeted federal services and programming through military organizations are needed.”</p> <p>8. There are also opportunities to tighten the writing. For instance, on p. 8 there are 3 sentences that imply how novel the study is in the Canadian context by line 31 (let alone several other mentions earlier on).</p> <p>Thank you for this comment. We have gone through and revised the manuscript for redundancies.</p>
Reviewer 2	Dr. Adam Vaughan
Institution	Simon Fraser University, Burnaby, BC
General comments (author response in bold)	<p>Many thanks for the opportunity to review this manuscript. Though I have a variety of comments for the authors to consider, much of my feedback is for clarification and/or minor in nature.</p> <p>1. With the low wordcount journals, the introduction/framing of the study tends to be the location where content is kept to a minimum. The first paragraph does a good job at framing up the need for the study. In the second paragraph, I got the impression that the authors were seeking to identify that high rates of relocation are likely to lead to 1) stress in children along with 2) difficulties accessing healthcare services when in a state of transition and 3) the variability of mental health services both within and between jurisdictions. I was expecting the authors to continue with these areas as the foundation for their current study. Rather, the next phase of the introduction highlights that there is a gap in understanding as much of the research is focused on US-based research which may or may not involve private healthcare services. My suggestion is to omit this portion of the introduction and move directly into the focus of this study which is to compare mental health service use to that of the general population and that the hypotheses are grounded in 1), 2) and 3) from above.</p> <p>Removed as suggested.</p> <p>2. I may have missed this in the manuscript, but I did not find where authors define ICES. Line 6 on page 4 was the first instance of this acronym that I could see.</p> <p>ICES is considered a legal name, rather than an abbreviation.</p> <p>3. What was the rationale behind matching four children from the general population to a child of the general population? That is, why four and not three or five?</p> <p>There is no guidance for sample size in matching ratios for cohort studies. We selected 4 comparators from the general population because this is the number typically considered best practice for case control studies and in this setting, provided an adequate number of events to study rare outcomes</p>

such as psychiatry visits while balancing the practicality of finding an adequate number of comparators in smaller geographic areas. We recognize that selecting the ratio based on case control studies is not rationale alone considering the differences in the two study designs.

4. Given the transient nature of the study group, were there any of the 5478 participants that relocated more than once to the same base or relocated to another base in Ontario during the study period? Related to this point is the actual study timeline.

All the military-connected individuals were beginning a new period of OHIP eligibility resulting from a military-related relocation from out of province or out of country. A study timeframe of three years was selected based on military tempo and the average amount of time an individual may stay at a particular posting. This may in fact be shorter or longer, depending on their occupation, rank, and other factors such as operational need; 88% of those in the military cohort had 3 years of follow-up. It is possible that within the three year time frame, the serving member(s) was relocated within Ontario.

5. On page 4 line 23, the authors identify the catchment timeframe for entry into this study. It may be useful to move the duration of data collection from page line 5 back to page 4 around line 23ish. The data duration sentence at the end of the first paragraph on page 5 seems out of place.

We included the duration of data collection within the paragraph referencing index dates on page 5 to ensure it relates to both the military-connected and matched cohorts. The suggested placement would be in a paragraph related to the military-connected cohort only.

6. What would happen, for example in a case where a participant began data collection at age 19 and was followed for three years? Would their data be truncated to fit the child/youth definition of 20 years?

Individuals were followed from study inception to one of the three identified end points (three years, relocation out of province, death). Age was not used to truncate the follow-up period.

7. Page 6 line 26. Median community income, though theoretically useful in a study such as this was not well presented. This is most certainly a relevant factor for access to healthcare services but nowhere in the introduction did I get the impression that socioeconomic status would be a relevant feature to consider for the current study. As an additional data source outside of the healthcare databases used by the authors, the Census is another data source and should be referenced in the Data sources subheading. In fact, income was only discussed at length in the limitations section where the authors state "Children and youth in CAF families may have better access to mental healthcare as the result of as their higher family income." Perhaps if there is a study from the general population that highlights the impact of socioeconomic status and access to mental healthcare, this could be included in the introduction. E.g., Reiss, Franziska. "Socioeconomic inequalities and mental health problems in children and adolescents: a systematic review." Social science & medicine 90 (2013): 24-31.

We do not believe that including a discussion of income is required in the introduction section of the manuscript, in particular given the limited word count. Income was included as a potential confounder and given the same

attention and level of detail as the other covariates, such as age, sex, and rurality. We have included a reference to the census data in the data sources section as suggested and included the suggested citation in the limitations section.

8. Given the nature of the paper and word count requirements, the results section was largely well-done. Much of the results are bound to the array of tables which inevitably requires the reader to flip back and forth between tables and the text.
No changes necessary.

9. In Table 2 the authors control for age, sex, region, in RR* and age sex, region, income, rurality in RR** but when it comes to Table 3 only age, region and income and age and region are accounted for? Was there a particular reason why only age and region were captured by both statistical tests? Table 4 had different control variables yet again with geography considered. Is “geography” the same as “region”? Line 52, page 5 states “geographic region of residence” which I am assuming is the same thing. Please clarify.

Clarified as requested in the table legends. All analyses included the same set of covariates: RR1 Age, sex, region; RR2 Age, sex, region, community income and rurality

10. It would be useful for the authors to expand on the last sentence at the top of page 6. It reads “In either interpretation, there is evidence that relocation disrupts access to specialists, as evidenced by lower use of pediatricians and longer time to first psychiatrist visit, and these are resources already known to be in shortest supply.” That is, what does it mean if GP access for CAF families is adequate for those children that require more specialized care? The authors mention capacity building in the “future directions” but the results of this study suggest that there could be some policy recommendations such as additional resources are needed for specialized healthcare for CAF families to access.

Additions to the discussion have been made to address this comment. Given the number of changes to this section, the text has not been copied over to the reviewer response document.